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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Additional Global Climate Cooling by Clouds due to Ice Crystal Complexity" *by* Emma Järvinen et al.

Anonymous Referee #1

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General comments

The paper presents a compilation of light scattering measurements obtained from a large number of aircraft campaigns, distributed globally, and they relate these measurements to ice crystal submicron complexity. This enables the authors to obtain estimates for the asymmetry parameter, a parameter of importance in NWP and climate modeling. They find from their analyses that the asymmetry parameter determination of 0.75 can be related to their complexity findings. This appears invariant with location and ice/cirrus formation, and the resulting scattering pattern results from the observed ice crystal complexity. As a consequence, this complexity expressed through the asymmetry parameter induces a not insubstantial-averaged further cooling effect not currently accounted for in climate models.

This is a largely well written paper, which links experimental results with theory and relates these measurements to ice crystal complexity and follows the theory through to an application in climate models. The paper provides nice results which deserve to be published, but the claim needs to be proven more rigorously with uncertainties attached to their estimates.

Major comments

1. The claim of the authors is that their measured PN angular scattering patterns are sufficient to determine the asymmetry parameter through some theoretical phase function that appears to fit through the data. This is not convincingly shown to be the case and appear to be eye fits at one single wavelength. There is no discussion in the text as to how the best fit to the measurements was statistically determined? Moreover, there are a number of extrapolations that could be used owing to the spread throughout the data, what uncertainty does this spread produce in the estimated asymmetry parameter values? There should be an uncertainty attached to their estimate of 0.75±? Once these uncertainties have been derived for the asymmetry parameter, the uncertainty in the SWCRE should be consequently determined.

2. The other wavelength of 0.804 um is only once shown, the same as Figure 5 should be shown but for 0.804 um using all models. Moreover, the eight-column aggregate shown at 0.804 um, is only just within the measured uncertainties at side scattering angles. This could be owing to the aspect ratio of the monomer columns not being sufficiently large and spaced out more than the compact model they show. The aspect ratio is also an important determinant of the asymmetry parameter as shown by Fu (2007), among others. It would be interesting to plot the approach of Fu (2007), to see if that treatment provides similar low values to those being estimated from the data.

3. The paper concludes that it is appropriate to apply the eight-column aggregate in climate and weather models. This is a rather significant claim as the model has only been tested at one single wavelength, at 0.805 um, it does not appear to possess the

correct absorption properties at side scattering angles for the possible reasons stated above. It is unclear as to how this model would fit observations at other wavelengths of importance, such as in the terrestrial window region, far infrared, and at more absorbing solar wavelengths, such as at 1.6 and 2.2 um. These wavelengths are also of importance in weather and climate modelling. The authors present no evidence to support their general claim.

4. A further point about Figure 5 also needs to be noted. Recent theoretical electromagnetic studies have shown that surface roughness, at scattering angles around exact backscatter, induces coherent backscattering, so the phase functions of surface roughened ice should not apparently be flat at exact backscattering angles, there ought to be some backscattering amplitude present. The authors are referred to the following paper for further information about this interesting interference effect, https://www.osapublishing.org/DirectPDFAccess/B8203150-AE8E-68E9-D2CB7062A1AB5EF8_385794/oe-26-10-A508.pdf?da=1&id=385794&seq=0&mobile=no . To compute the phase functions, the authors use a database which probably applies the improved physical optics approximation, in that multiple scattering is not included, so surface roughness is approximated by some geometrical treatment such as facet tilting to smooth the phase functions that appear in Figure 5. As a consequence of this, one could argue that the phase functions presented in Figure 5 are incorrect. Of course, owing to the asymmetry parameter being largely determined by diffraction, its derived value will not be much affected by this backscattering amplitude. However, this still does need to be noted in my opinion to encourage inclusion of multiple scattering in calculating the phase functions, especially if they are to be used for lidar applications at visible wavelengths. However, to obtain more representative phase functions, the backscattering amplitude could be added on to the phase functions presented in Figure 5. There is a parameterization that the authors could use to do this as explained in this paper https://www.osapublishing.org/oe/abstract.cfm?uri=oe-24-1-620, where IGOM is corrected using the estimated amplitude obtained from electromagnetic calculations.

5. Also, for some reason, the authors do not cite papers prior to 2010, there are some, but these are few and far between and tend to be their own. This needs to be corrected.

Minor comments now follow:

1. In the abstract, the averaged asymmetry parameter of 0.75 is determined at the wavelength of?

2. Introduction line 15, similar results by Ulanowski et al., (2006) and Ulanowski et al. 2014 were also reported.

3. Introduction line 16, representations of ice crystal surface roughness via facet tilting were also added prior to 2008 by Macke et al. (1996)[https://journals.ametsoc.org/doi/pdf/10.1175/1520-0469%281996%29053%3C2813%3ASSPOAI%3E2.0.CO%3B2], Yang and Liou (1998) [Single-scattering properties of complex ice crystals in terrestrial atmosphere, Contr. Atmos. Phys., 71, 223–248, 1998], Baran et al, (2001)[https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.49712757711], Baran and Francis (2004)[https://rmets.onlinelibrary.wiley.com/doi/10.1256/qj.03.151], Sun et al. (2004)[https://www.osapublishing.org/ao/abstract.cfm?uri=ao-43-9-1957. There are of course others.

9. Page 2, discussion on polarization, line 2, The same was also shown by Baran and Labonnote (2006) [https://www.sciencedirect.com/science/article/pii/S0022407305003699] in regards to polarization.

10. Page 3, line 15, replace “in” by “on”.

11. Page 3, line 25, perhaps, the word “the” needs to be incorporated before “discrete dipole”.

12. Page 3, line 34, insert the word “to” before “as”. . .

13. Section 2.2, in the discussion on the PN being used to determine the angular scattering functions, there is no explanation or discussion as to how shattered artefacts were removed from the analysis. Please could you insert this, otherwise, we may be led to believe that those functions could be more pertinent to shattered ice and so will provide low asymmetry parameter estimates.

14. Section 2.4, perhaps save space by compiling the list of campaigns into a table? This improves readability.

15. There are many campaigns dating back to before 2010, how did the authors make sure that the PSDs were treated consistently into one database from the variety of differing microphysical probes?

16. Page 5, line 23, suggest replace "to" with "for" . . . the analysis. . .

17. Section 2.5, please add a description of the current ice optical parameterization used in ECHAM-HAM. It is often referred to but unknown as to what it actually is.

18. Page 7, line 6, suggest insert the word "to". . . a change. . ..

19. Page 7, line 10, comma after aggregates?

20. Page 8, there are a whole list of studies that predate 2010 in showing that flat featureless phase functions best represent angular short-wave measurements obtained from above ice cloud such as Doutriaux-Boucher et al., (2000)[https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/1999GL010870], Labonnote et al. (2001)[https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2000JD900642]. A more recent paper by Letu etal. (2016) [https://www.atmos-chem-phys.net/16/12287/2016/] uses comprehensive PARASOL short-wave reflectance data to show the same.

21. Page 8, line 16, Again, there are many papers that predate 2013, please cite a representative sample.

22. Page 9, line 5, typo "sdiscussed".

Figures:

Fig. 1 penale-> panel.

Fig. 2 difficult to distinguish purple from red, suggest changing purple to green.

Table 2. Please also insert the percentage of the total particle population rejected owing to shattering.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-491/acp-2018-491-RC1-supplement.pdf

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